volcanic ejecta, by the analogy of the blast furnace, in which the same materials in the same proportions do not even in any one furnace, or at all times, produce identical slags.

What is Mr. Forbes's reply? That the *intention* of the iron master is to produce slags always the same, as the indication that the furnace is working well.

Doubtless the intention and desire of the iron-master is to produce good iron, and at all times as nearly as he can such a slag as indicates that he is doing so. But, as a matter of fact, he is not able to reach this. He can only approximate to constancy in the chemical or mineralogical constitution of his slags, which are never identical, even for short periods. Is this substitution of the intentions of the iron-master for the actual facts of the blast furnace slags, on Mr. Forbes's part, worthy of the candour of the searcher for truth; or does it not rather resemble the dialectic wriggle of the advocate?

Complete identity between any two rocky masses, ejected or otherwise, can only exist where the same elements in the same proportions are combined in the same way, and in the same molecular aggregation. If the mere presence in greater or less proportion in the mass, of certain crystallised minerals in any variable proportion, such as felspar, pyroxene, or leucite, in the magma of lavas, were enough to constitute identity, then nearly all the known rocks of the world, crystalline, igneous, and sedimentary, might be viewed as identical, for all consist of a few elements and of a few prevailing simpler minerals.

While still seeming to maintain his original statement, Mr. Forbes now substitutes for *identity*—a great *similarity* in all volcanic rocks. Further discussion is therefore needless—nor indeed would discussion of my views as to volcanic heat, &c., lead to any good result—with a gentleman whose notions of scientific method are such, that after six months' consideration he holds any distinction between hypothesis and theory to be mere hair-splitting, and whose notions of physico-mechanics are of that confused character, that he views pressure and work to be quite the same, and that it is matter of indifference whether we talk of "pressure converted into its equivalent, heat," or of work transformed into heat.

Would Mr. Forbes enlighten your readers by stating in figures what is the equivalent in heat, of the pressure of a weight of ten pounds, resting upon a rigid level plane?

Were Mr. Forbes of any real authority upon volcanic subjects, there might have been more ground for his sweeping and anticipatory condemnation of my views as to volcanic energy, which, however, in that case, he would never have uttered; but on looking down the list of his published papers, I do not find any treating of vulcanology simply, nor am I aware that he has ever enlarged the boundaries of our knowledge in that department by a hair's breadth.

Mr. Forbes appears to think that chemists, mineralogists, and geologists are the sole arbiters of all questions as to the nature and origin of volcanic heat and energy. Whatever they may have done to add to our knowledge of the visible and tangible phenomena of volcanic vents or cones, they have as yet contributed really nothing to discovering the nature and origin of volcanic heat itself, if we except some valuable negative evidence drawn from the gaseous emanations by chemists of late years, subversive of the older theories of the chemical origin of volcanic heat, still not quite extinct. It is much more to the physicist and theoretic mechanician dealing largely with the *physique du globe*, that we must look for further light, and whose province it will be to decide when the right key shall have been found to that enigma of ages, the true nature and origin of volcanic heat and energy.

I am done, sir, with this controversy, unwillingly entered upon, not in irritation, as Mr. Forbes states, but because I felt justified in protesting against new and I believe important views being obscured *in limine*, by objection based only on error.

My paper containing those views will ere long be before the world. My 100 separate copies (as author) from the "Phil. Trans." are already in the hands of or on the way to many men of science. The volume itself of the "Transactions" will no doubt appear before the end of the year, and to the verdict of the real men of science of the world, versed in the subject and competent to judge of it, I leave the result.

London, Oct. 6 ROBERT MALLET

On the Equilibrium of Temperature of a Gaseous Column subject to Gravity

FROM Mr. Clerk-Maxwell's reply to my note on this subject which appeared in your columns a short time since, it would appear that he does not profess so much fully to explain the difficulty suggested by me as to show that it is capable of explanation, referring your readers to his other works for further information. I would not, therefore, have troubled you further on the subject had it not occurred to me on reading Mr. Maxwell's letter that I could state the case in such a way as to render clearly apparent the grounds for taking different views on this point.

Let a vertical column of gas, subject to gravity and in a state of equilibrium as to pressure and temperature, be divided by a horizontal plane P into two parts, A above and B below.

In the time Δt let a mass M_1 of particles pass in their free course from A to B, and a mass M_2 from B to A.

Let the portion of A from which the particles composing M_1 proceed be called the upper stratum, and the corresponding part of B the lower stratum, then the following consequences may be deduced:—

1. From the equilibrium of density

$$M_1 = M_2$$

2. From the equilibrium of temperature the amounts of work in M_1 and M_2 while passing through P are equal.

3. From the effect of gravity the work in M while in A reckoning from the commencement of the free course of each particle composing M_1, is less than at P, while that in M_2 is greater.

4. Whence it follows that of the two equal masses M_1 and M_2 in the upper and lower strata respectively M_1 contains less work than M_2.

5. The work in M_1 while in the upper stratum reckoned as before, is the same as that of any other equal average mass in that stratum, and the same is the case also of M_2.

6. The average amounts of work in equal masses in the two strata, and the consequent temperatures of the strata are unequal, the lower stratum having the higher temperature.

I suppose Mr. Maxwell would deny the truth of statement (5). I presume he would argue as follows:—

"Of all the particles in the lower stratum which in the time Δt have at the commencement of their free course a velocity and direction such as would take them through P, gravity in selecting those which compose M_2 excludes those whose velocities are insufficient to overcome the effects of their weights, while in forming M_1 particles of low velocity are selected (included?), which, but for the effects of gravity, would not have cut P in their free courses, consequently the particles in M_1 have an average velocity less than that of the upper stratum from which they come, while the particles of M_2 have a greater average velocity than that of the lower stratum, and consequently the inequality of the average velocity of the particles in the two strata cannot be inferred from the inequality of the average velocities of the particles composing M_1 and M_2 while in those strata."

This argument, therefore, assumes the theory that in a given mass of uniform temperature there are particles moving with every velocity from nothing upwards to a certain limit, and mixed in certain proportions. That this is actually Mr. Maxwell's view I own I might have remembered, but I suppose I overlooked it from an impression in my own mind that the molecular motion was to be regarded as being of a planetary (or in the case of gases a cometary) nature. That in masses of the same temperature velocities were to be regarded as practically uniform, except in so far as affected by the distance of the particles apart, and that the so-called impacts of particles were more properly to be regarded as perihelion passages of bodies moving among each other in hyperbolic orbits. If this view is the more accurate one, then obviously the argument which I have assumed that Mr. Maxwell would use, falls to the ground.

Is there no possibility of testing the nature of the thermal equilibrium of a column of still air? The result would at any rate throw an unexpected light on the nature of molecular motion.

Graaff Reinet College, July 19 F. GUTHRIE

The Sphygmograph

DR. GALABIN, in his letter published in your last number, criticises my explanation of the cause of the small wave in the first part of the sphygmograph trace, which he calls the tidal wave. In his criticism he does not take into consideration the hæmodromograph traces of Chauveau, on which my explanation

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is entirely based, and without a reasonable interpretation of which no explanation can be considered satisfactory. The hæmodromograph trace proves that the "tidal wave" of Dr. Galabin has a shock origin, as I have shown in the "Journal of Anatomy and Physiology" (Nov. 1872), and that the dicrotic wave is its resulting tidal wave.

Dr. Galabin appeals to the "tidal wave" in the trace from the artery at the foot, in proof of his explanation; I have taken many from that locality, and find that the tidal wave is never represented at all (as my explanation requires), for it is thrown so far back that it becomes blended with the primary rise.

My explanation of the details of the cardiograph is questioned, because my tracings are said to have been taken with "a lever moving on a pivot, and balanced between two springs." Such was undoubtedly the case in my cardio-sphygmograph observations, but not in my paper on the cardiograph trace, when the instrument employed was, what Dr. Galabin recommends, the ordinary sphygmograph, applied to the chest-wall.

As long as Dr. Galabin has not full faith in the reliability of the sphygmograph and its indications, it is almost impossible to maintain an argument with him, for it is hardly worth discussing points which may be only the results of instrumental imperfections. These are now understood, and can be easily eliminated.

A. H. Garrod

Venomous Caterpillars

The caterpillars mentioned by R. Benson in your paper of August 14, are not at all uncommon in Calcutta. One day my little girl was brought to me with what appeared to be a good sized hairy caterpillar under her arm, and crying as if in pain, and on my trying to remove it in a hurried way, I discovered that it was nothing but a mass of small hairs. The child had put her arm into an empty tub on the inner edge of which the caterpillar was crawling. As soon as she pressed it, she started as if she had been stung. All the servants crowded round the child and pointed to their heads, but as I was not a proficient in their language I could not make out what they meant. I tried to do what I could with my fingers to remove the hairs, but this seemed very painful, and the swelling round about kept increasing. The ayah, however, soon appeared, attracted by the child's crying, and seemed to know what was to be done. She got some of my hair, made a kind of small brush of it, and gently passed it over the injured part. In a few moments the hairs were all removed, and nothing was left but a white blister. This remained for two or three days and then subsided. In the Calcutta schools the boys call these caterpillars "woolly bears," and if stung by them ask for "a head," and a few rubs soon removes the disagreeable appendages.

C. H. C. B.

Calcutta, Sept. 9.

Harmonic Echoes

Lord Rayleigh's notes on Harmonic Echoes recall to my recollection a little experience which I had in hearing what I supposed to be overtones reflected.

I have frequent occasion to cross a portion of an open public park in which there are few trees. When any sharp sounds are heard in the neighbourhood, as, for instance, the sound of the rod in the beating of carpets in a field near at hand, curious responses to the blows of the rod are heard, and these responses or echoes have not the same pitch as the originating sound. I was puzzled for some time to account for this echo in an open park, with almost nothing above the level of the grass but the iron railings, till I satisfied myself, by occupying various positions, that the echoes were reflections of sound from these narrow fences. But why the difference in pitch between the originating sound and the echo? This, I concluded, *might* result from the overtones of the sound being reflected from the thin iron bars which constitute the railing. It was also observable that it was only the sharp sound emitted by the beating rod which was echoed, and not the dull sound arising from the carpet when struck. The hands struck sharply together will also cause an echo from the fences, which is higher in pitch than the sound of the clapping hands. It would be very interesting to experiment on this point by sounding, at a proper distance, notes of known pitch before narrow, upright, or horizontal bars, and then ascertaining the pitch of the echo, and the relation of the latter to the size of the reflecting surface.

W. J. M.

Glasgow

It appears tolerably well established that harmonic echoes are selective echoes; that is to say, echoes which, from whatever cause, select and return one of the harmonies of the original without the fundamental.

It may perhaps be found that there are other selective echoes than the harmonic kind. In one of the galleries of the very large parish church of Monkstown, co. Dublin, the sound of S is heard with peculiar intensity, both in the singing and in the responses. This is not an echo, but it may perhaps be a fact of the same kind with selective echoes.

Old Forge, Dunmurry

Joseph John Murphy

Carbon Battery Plates

Could you oblige me with information (or state where it could be obtained) respecting the process of manufacture of hard carbon battery plates, as I have some experiments on hand which necessitate the manufacture of plates of a peculiar shape, and I can neither get them made nor obtain sufficient information to enable me to make them well.

Warrington

T. W. Fletcher

Brilliant Meteor

On the evening of September 7, at about 9.7 P.M., while walking in a northerly direction in one of the streets of Tiverton, I saw a very large and brilliant meteor slowly descend from east to west, but in an almost vertical direction. The sky was almost entirely covered with a thin veil of cloud, which obscured the stars, so that I was not able to note its course with reference to them; but the altitude of the point at which it first appeared was about 45°, its path was inclined to the vertical at an angle of about 5°, and it disappeared behind a roof at an elevation of about 20°, at a point about 90° to the north of the moon which could be seen through the clouds. The light of the meteor was greenish and flickering, and far exceeded in intensity that of Venus when at her maximum brilliancy, but I could not see any train.

T. Perkins

Reading School

NORTHERN LIMIT OF PHANEROGAMIC VEGETATION

Captain Markham has most kindly presented to the Herbarium of the Royal Gardens, Kew, a small but very interesting collection of plants brought back by him from his recent Arctic voyage. Amongst them are four specimens which he obtained from Dr. Bessel, who collected them in lat. 82° N., the most northern position from which any phanerogamic vegetation has hitherto been procured. The locality appears to have been on the east side of Smith's Sound. The species are *Draba alpina*, L.; *Cerastium alpinum*, L.; *Taraxacum Dens-leonis*, Desf. var.; *Poa flexuosa*, Wahl.

Jos. D. Hooker

THE WEALDEN BORING

The readers of Nature will be interested in learning that the lowest beds now reached by the Sussex boring are not Wealden, but of marine origin; that the most distinct of the shells yet examined by me is a Lingula, that it is *Lingula ovalis*, a shell of the Kimmeridge clay. The specimens which contain it were placed in my hands by Mr. Peyton, with Mr. Willett's consent. We are, in fact, already below the Wealden, in the pelagic sea-bed far from its ancient shore.

J. Phillips

THE NEW MARINE ANIMAL FROM WASHINGTON TERRITORY

At the meeting of the British Association in 1872, I exhibited before Section D specimens of some long white bodies resembling peeled willow-wands, which I had received from Barraud's Inlet, Washington Territory, with the information that they were the "backbones of a fish." Subsequently I published what intelligence I

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